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S.P.R. MEETINGS

(open to Members of the Society and their guests)

26 January at 3 p.m.	L. J. BENDIT, M.D., D.P.M., on 'Perceptivity and the Psyche : Some Considerations'
11 February at 2.30 p.m.	C. W. K. MUNDLE, M.A., on 'Precognition'
28 February at 6 p.m.	ERIC CUDDON on 'The Technique of a Certain Clairvoyante'
9 March at 3 p.m.	JAMES LEIGH on 'Psychical Research and Occultism'
23 March at 6.30 p.m.	S. G. SOAL, M.A., D.SC., on 'The Psi Faculty : A Personal Record'

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THE PARAPSYCHOLOGY LABORATORY AT DUKE UNIVERSITY, AND THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

SOME IMPRESSIONS

BY D. J. WEST, M.B., CH.B.

I. THE PARAPSYCHOLOGY LABORATORY

IN ANY field of study it is desirable that workers should meet to get acquainted with each other and exchange ideas. In parapsychology there are special reasons why this is particularly important. We are far from having a strictly repeatable experiment. We cannot say to a would-be investigator, 'do this and you will get a result'. Many have tried and failed. We still depend upon the integrity and competence of such observers as are able to obtain positive results. It is not understood why some experimenters are successful and others are not ; but it is suggested that their personality, their attitude to the tests, and the way they treat their subjects are important factors.

Over the years the Parapsychology Laboratory has poured out a constant stream of apparently reliable reports of successful E.S.P. and PK experiments many times greater in volume than anything other groups have been able to accomplish. It was with the idea of trying to find out what magic is responsible for the unusual success at Duke that I journeyed there and stayed four weeks out of the seven that I spent in America in the autumn of last year. This central problem is the main theme of my account, and for the purpose I shall need to describe to some extent the background and organization of the Parapsychology Laboratory, and to neglect giving any detailed review of particular pieces of research. These can in any case be studied much better in the original reports.

Some of the early work at Duke has been criticised on the grounds of incomplete precautions against sensory leakage. Such criticisms may have been relevant fifteen years ago but they were

not applicable to anything I saw at Duke. The impression I received as a result of getting to know the Duke experimenters was one of complete sincerity and straightforwardness. I feel sure that reports in the *Journal of Parapsychology* of experiments conducted at Duke are fair and full descriptions of what takes place, and the reader can be confident that the conditions set down in print were meticulously and competently maintained throughout. I found that some of the Duke experimenters were aware that in these respects many European experiments were inferior to their own. There was an interesting notice, pinned to the wall of one of the experimental rooms, which began: 'To aid the experimenter in securing his work against loss through inadequate precautions'. The following extracts were among the provisions included in this notice:

'Length of the experiment should be specified before the experiment is started.'

'A plan of procedure should be submitted in advance and filed with the record librarian . . . '.

'Officially numbered record sheets . . . should be used for all experiments . . . '.

'Recording methods should be worked out so as to rule out the possibility of motivated error . . . '.

'The use of ink is recommended . . . there should be no erasing.'

'There should be independent checking, independent evaluation, and independent computation of all data . . . '.

The results obtained in the Parapsychology Laboratory may be exceptional, but the situation there has no parallel anywhere else in the world. The laboratory is sheltered, materially and spiritually, by a great University. Rhine first came to Duke to work under McDougall in 1927, when the latter was engaged in forming a Department of Psychology in what was then a brand new university. E.S.P. research began in 1930, but it was not until 1940 that Rhine became director of a Parapsychology Laboratory employing workers independently of regular psychology appointments.

The Laboratory occupies a suite of fifteen rooms in one of the University buildings, and is financed partly by the University and partly by contributions from private donors. The entire revenue is used for sponsoring research—a very different situation from that which obtains in organizations privately maintained. Even the cost of printing reports in the *Journal of Parapsychology*, a quarterly ably managed by Mrs Dorothy Pope, is covered by revenue from readers' subscriptions.

The arrangement at Duke enables a group of trained research-

workers to concentrate on parapsychological experimentation as a full-time profession. The Laboratory is well equipped with such apparatus as dictating, transcribing and duplicating machines, various automatic shufflers, a room-to-room signalling system for use in synchronising guessing experiments, and the famous automatic dice-thrower, which takes a complete photographic record of the fall of the dice. More important than these mechanical aids, however, are the benefits derived from working in consultation. In a subject in which lecture courses are virtually unknown, the presence of experienced investigators, able and willing to help, is invaluable to the beginner.

At the time I was there, the staff present at the Laboratory consisted of the following: The Director of the Laboratory (J. B. Rhine, B.S., M.S., Ph.D.); three full-time permanent research workers (J. G. Pratt, A.B., M.A., Ph.D., Betty M. Humphrey, A.B., Ph.D., Elizabeth A. McMahon, A.B., M.A.); a part-time research associate (Louisa E. Rhine, B.S., M.S., Ph.D.); four research assistants, (Mrs Esther Foster, Mr Pope Hill, Mr Jack Kapchan, Mr Malcolm Turner); and the Managing Editor of the *Journal of Parapsychology*, (Mrs Dorothy Pope). There are also two secretaries and a clerical assistant.

From reading many popular accounts of 'Dr Rhine's experiments' one tends to get a false impression of a one-man establishment. It comes as a surprise to the visitor to discover a considerable divergence in the outlook, methods, and function of the different individuals who make up the Laboratory. While it is true the Laboratory owes its very existence and a great deal of its character to the enterprise and perseverance of Dr Rhine, it is nowadays his staff who carry out the actual business of experimenting. Most of Dr Rhine's working day is spent in administrative duties and in answering letters from his innumerable correspondents all over the world. Entertaining the endless stream of visitors to the Laboratory and undertaking lecturing and money-raising expeditions account for much that would otherwise be his private life. The only time left over for research is spent in advising and helping in other peoples' projects. Dr Rhine is a buoyant, enthusiastic personality, undismayed by opposition, criticisms, and set-backs in research; although at times he is impatient of the snail's-pace advance of our knowledge. His zeal and energy are boundless, and his utter sincerity undoubted even by those who criticise his methods. He believes that parapsychological findings have great practical importance, that their acceptance will carry with it an acknowledgement of the high place of man in nature and bring about a corresponding improvement in human relations.

Dr Louisa Rhine shares her husband's devotion to the subject.

At present she is working on a project far removed from the card-guessing and dice-throwing experiments which have made the Parapsychology Laboratory famous. She is collecting accounts of spontaneous psychic experiences and studying them for hints as to determining conditions, which she hopes may be reproduced and tested in experiments.¹ Anyone who has had such experiences, or knows of people who have had them, can assist in this research by writing direct to Mrs Rhine. Since it is not the aim of this study to produce evidence, but rather to seek for ideas, the cases need not have corroborative documentation, and it matters not in the least whether or not the reporter is a person of consequence or whether the story sounds plausible, provided it is told with frankness and sincerity, and the background circumstances are described in detail.

Apart from the Rhines themselves, the senior member of the staff is Dr J. G. Pratt, who has been at the Laboratory since 1932. His main concern at the moment is a statistical investigation of card-calling patterns, in which project he is ably assisted by Mrs Esther Foster.² Dr Pratt does not regard himself as particularly successful at collecting positive E.S.P. data, and tends to leave this task to others. He attributes the success of the historic Pratt-Woodruff³ tests to the personality of his colleague. In addition to his own investigations, Pratt is often called upon to perform statistical analyses on work done by experimenters outside the Laboratory, as well as to train new workers in the increasing complexities of parapsychological techniques.

Dr Pratt is a very patient worker, willing to pursue for month after month a series of tedious analyses, slowly distilling from apparently arid data a yield of interesting and important information.

Glancing through the other names on the list of the Laboratory's research staff, one finds three that will not be familiar to British readers. The first, Mr Pope Hill, is a young man who was successful as a high-scoring subject in some informal experiments conducted by William Russell some ten years ago. He has regained his interest in the subject, and is now at Duke hoping to devise tests which will show up his latent psychic abilities. At the present

¹ See *Jnl. S.P.R.*, vol. 35, no. 652 (May 1949) pp. 66-7.

² née Bond. See her report, 'General E.S.P. with a Group of Retarded Children', *Journal of Parapsychology*, vol. 1, no. 2 (June 1937) pp. 114-22.

³ See J. G. Pratt and J. L. Woodruff, 'Size of Stimulus Symbols in E.S.P.', *Journal of Parapsychology*, vol. 3, no. 2 (Dec. 1939) pp. 121-58. The noteworthy feature of these experiments is the rigour of the precautions whereby fraud, sensory leakage, and other counter-hypotheses were excluded.

time he is enthusiastically pursuing some object-reading experiments, but he is impatient of the methods of statistical appraisal, which so far have not demonstrated any consistent significance in his results.

Mr Jack Kapchan is one of those experimenters, only too familiar in Britain, who, however many times they try, never get positive results. The explanation suggested by some experimenters is that his somewhat formidably extravert personality inhibits subjects. Kapchan himself tends to be understandably critical of methods which can only be carried out by certain mysteriously privileged experimenters.¹

Mr Malcolm Turner is relatively a newcomer, a young student whose work in the Laboratory has so far been mainly learning and assisting in statistical computations.

So far I have named seven research workers without mentioning one who is collecting fresh positive E.S.P. data. At the moment this function, which might be considered the most important of all, falls to Dr Betty Humphrey and Miss Elizabeth McMahan. The senior of the two is Dr Humphrey. Her interest in the subject dates back to 1936, when she was a freshman at Earlham, a Quaker college. Under the guidance of Dr John Clark, a teacher of philosophy, there was formed a small group for trying some of the E.S.P. tests published by Dr Rhine. Betty Humphrey joined the group, but was at pains to make it clear that in so doing she was not committing herself to a favourable interpretation of the claims put forward by the Duke workers. In 1940, in spite of her critical reserve, she was offered, and accepted, an assistantship under Dr Rhine. Now she is a star experimenter, having published positive findings in almost every branch of experimental parapsychology. Dr Humphrey is a practically minded person. She gets an idea for an experiment, and goes right out and tries it, thereby achieving results in cases in which a more analytically circumspect investigator would have hesitated to take the plunge. Her research projects at the present time are chiefly concerned with the resolution of E.S.P. data into high and low-scoring groups by separating the subjects according to various personality assessments.² The vistas opened up by these highly interesting developments are endless, but what has so far been done is no more than a modest beginning.

¹ Since writing this I hear that Mr Kapchan has obtained some significant differences between subjects' E.S.P. performances by sorting out aggressive and non-aggressive groups.

² Betty M. Humphrey, 'Success in E.S.P. as related to Form of Response Drawings', *Journal of Parapsychology*, vol. 10, no. 2 (June 1946) pp. 78-106; vol. 10, no. 3 (Dec. 1946) pp. 81-96; also 'The Relation of E.S.P. to Mode of Drawing', *ibid.* vol. 13, no. 1 (March 1949) pp. 31-47.

Miss Elizabeth McMahan was introduced to the subject by her teacher, Dr Burke Smith, who was at one time a member of the Parapsychology Laboratory. On his recommendation she came to Duke in 1943, graduating in psychology in 1946, after which she became a full-time member of the Laboratory research staff. Miss McMahan says she enjoys the actual process of seeing subjects and carrying out experiments with them, a fact not surprising in view of the cheerful, confident way in which she approaches the task. Since 1945, she has published a variety of reports on E.S.P. and PK projects, and has proved herself successful in obtaining positive results. Her chief experimental preoccupation when I was at Duke was in precognition tests in which there were time intervals between the registration of the 'guess' sequence and the checking against a future 'target' sequence derived from figures in a newspaper weather report.

After this brief account of the Laboratory organization, we are in a better position to appreciate the results obtained there. It would be a great mistake to suppose that experiments at Duke invariably give positive results. I have already remarked upon the existence of unsuccessful experimenters. One such, William Birge, a very keen worker, recently transferred his attentions to orthodox psychology because he could not obtain conclusive results from any data he collected. William Davidson, who is now studying for a medical degree, worked for four years on experiments designed to test the effect of the human will upon the behaviour of rats, but it is considered that more and better work is needed in this field before publication is desirable. In my view it is unfortunate that every major experiment does not receive at least a brief mention in the *Journal of Parapsychology*. Readers are liable to receive a wrong impression if they only hear of positive results, a loophole is left for criticism on the grounds of unfair selection, and other experimenters may without knowing it duplicate methods already attempted and found to be unsatisfactory.

In earlier years Duke was famous for its star E.S.P. subjects, who, like A. J. Linzmayer and George Zirkle, produced phenomenally high scores over short periods, or who, like Hubert Pearce, Sarah Ownbey, and Margaret Pegram, were able to keep up consistently positive scores over long periods. Britain has never had any famous high scorers, and only three really persistent scorers.¹ In recent years the Parapsychology Laboratory has had

¹ G. N. M. Tyrell's subject Nancy Johnson and Dr Soal's two subjects Basil Shackleton and Gloria Stewart.

no star subjects whatsoever, so that in this respect, at least, their contemporary results are not superior to our own.

It was never claimed that star subjects could continue their performances indefinitely. Sooner or later the capacity of each one was observed to decline, just as in the case of the British subject Basil Shackleton. It is curious, however, that at Duke new subjects have not come forward to take the place of the old stars. There seems to be no completely satisfactory explanation. Critics have pointed out that in the early work at Duke, when the more striking successes were achieved, the conditions were on the whole considerably more lax than in the experiments at the present time, when there are no star subjects. It is impossible, however, to dismiss all the early work in this fashion, since some of it (e.g. Pratt's distance tests with Pearce and his experiments with the special subject Mrs M.) seems to have been satisfactory as regards the exclusion of sensory leakage. Dr Rhine considers that an important factor lacking in experiments today is the first flush of enthusiasm of the early experimenters. Dr Humphrey and Miss McMahan point out that in their experiments interest is not centred upon individual performance, but upon data obtained from groups of subjects, and it may be that under such conditions there is no stimulation for exceptionally high scores. Since they do not systematically re-test all their subjects, they have no means of knowing how many persistent scorers may have passed through their hands. Dr Soal might well be shocked at such nonchalance, to say nothing of Dr Pratt, who would dearly love to find another Mrs M.

The successful E.S.P. experiments reported from Duke today (under which heading are included experiments in clairvoyance, precognition, and pure telepathy) are always concerned with groups of subjects rather than with individuals, and the margin of success achieved, though statistically significant, is not as striking as in the old days of the star subjects. In many cases the data appears to show no significant deviations from chance expectation until it has been divided up into high and low scoring portions according to one or other personality rating. This is not in itself any reflection upon the validity of the recent work, since small deviations, if consistent, soon become conclusive, but one needs to keep in mind the characteristics of the results at present obtained. While it is true that nothing like these results have appeared in Britain, it is also true that relatively few attempts have been made here. The division of subjects into high and low scorers according to attitude and personality measurement is entirely an American innovation.

One field of work in which the Duke experimenters have had for

a long time a virtual monopoly of positive results is PK¹—the extra-chance effects observed in 'willed' dice throws. Like Dr Thouless before me,² I took part as subject in PK experiments while a visitor at Duke and produced extra-chance scores which were at least highly suggestive, and contrasted strikingly with the completely null results of the dice-throws in which I had previously engaged at the S.P.R. A description of these experiments will, it is hoped, appear in a later issue of this *Journal*.

The PK experiments have been the subject of considerable criticism. Last year Mr Denys Parsons read to a meeting of the S.P.R. a lengthy critique in which he expressed complete disbelief in the Duke conclusions, and some years ago I myself wrote a brief review of the PK experiments up to date³ which contained some critical comments. In his critique, Mr Parsons pointed out that no answer to these comments had been forthcoming from Dr Rhine. I was amused to discover that a devastating reply had in fact been written by Dr Humphrey, but she had not sent it to the S.P.R. because my conclusion had been on the whole favourable, and she did not think it worth while to take time off from research to engage in controversy on relatively minor points. In her reply Dr Humphrey showed that specific answers to most of my criticisms were contained in the published reports, which I had obviously not studied sufficiently carefully. I realise now that I do not know enough about the research to voice a useful opinion on the interpretation of the Duke findings, though I feel no doubt that their observations are genuine.⁴

While it is clear that at present the Duke results in E.S.P. and PK experiments are not so very different from those obtained elsewhere as has often been supposed, there is no doubt that on the whole they have had an outstanding record of success, and it is interesting to speculate upon what factors may be responsible.

The obvious advantage of laboratory facilities and full-time workers have already been emphasised. Another, and probably very important factor, is the friendly atmosphere in which experimenters work at Duke. The professional and social life of the Duke workers are inextricably intermingled, and there are many inter-related friendships, to say nothing of marriages. Seeing

¹ But see Laura Dale's excellently designed experiment, reported in *Jnl. A.S.P.R.*, vol. 40, no. 3 (July 1946); also the work of Dr Knowles and Dr Thouless in this country.

² See *Jnl. S.P.R.*, vol. 35, no. 650 (Jan 1949).

³ *Proc. S.P.R.*, vol. 47, pt. 170 (1945) pp. 281-90.

⁴ While at Duke, and with the kind help of Mr Malcolm Turner, I made copies of a considerable number of original score sheets of PK experiments, and brought these back with me for use in the further study of PK problems upon which Mr J. Fraser Nicol is at present engaged.

them together, as I did one week-end, picnicking round a camp fire, they looked like one big family party. To a visitor, used to the disputes and controversies which appear to beset psychical research in most parts of the world, the Duke set-up appears a phenomenon in itself.

Dr Rhine likes to say that the Laboratory is run by anarchy, by which he means that formalities, regulations, and committees are as far as possible avoided. He believes that to get the best results experimenters should have great freedom in their choice of project for investigation. If new recruits prefer to go their own way without guidance, Rhine would rather they did so and learned by experience than that they should feel themselves forced into an unattractive line of investigation. This may to some extent account for the apparent lack of an integrated research policy at Duke, which some critics have attributed to lack of foresight.

Rhine's methods of recruiting new experimenters are probably important. He believes in enlisting them young, at college age, when enthusiasm runs high and receptiveness to new ideas and methods is at a peak. Malcolm Turner, for instance, who was the newest member of the Laboratory when I was there, was at college in Atlanta when he first wrote to Dr Rhine a letter displaying some keenness and knowledge of the subject. In Turner's own words, 'Dr Rhine's reply just about sold me.' In February this year he paid a visit to the Laboratory and Dr Rhine indicated that if he were a student at Duke, some part-time work might be found for him in the Laboratory. With this encouragement, he soon accomplished the necessary transfer, and now Dr Rhine has one more able young worker.

In Dr Rhine's opinion the provision of a free, friendly atmosphere in which the experimenter can work contentedly is important, and paves the way for the still more important factor, the establishment of correct experimenter-subject relationships. The Duke recommendations for getting the best results from subjects have been admirably set forth in Dr Rhine's book *New Frontiers of the Mind*.¹ Briefly, the experimenter's approach to the subject should be natural and friendly, avoiding a formal or intimidating manner. Dr Humphrey tells me she talks to her subjects a little while to put them at ease before beginning tests. She likes working with younger people, especially matter-of-fact students who are used to experiments, in preference to older persons, particularly those who believe they have special powers and feel themselves on the defensive in an unfamiliar situation.

The experimenter should try to inspire the subject with both confidence and interest in the task before him. The first of these

¹ London, Faber, 1938.

requirements is helped by the tradition of success which has been built up at Duke, so that no-one is surprised when an experiment succeeds. Interest is added to dull experimental situations by introducing a competitive element, or giving the subject some insight into the problem which his results will help to solve. Best results are obtained when the subject is feeling in the right mood. For this reason, Dr Humphrey and Miss McMahan both like to have experiments set up in readiness for volunteer subjects who call at the Laboratory at any moment when they happen to have time to spare and feel they would like to try something. When Dr Humphrey has a subject, she tries to fit in with his mood, winding up the preliminary conversation and beginning the experimental trials at the moment he feels ready. Fatigue and boredom in either experimenter or subject are bad, so that short sessions, and not too many trials per subject, is the ideal.

I asked Dr Humphrey for her views on why so many experimenters are unsuccessful. She replied that among the unsuccessful experimenters she had known at Duke she had almost always been able to put her finger upon some flaw in their approach which might account for their failures. One experimenter seemed to kill subjects by kindness, creating tension by being over-attentive. Another was too ambitious and rushed his subjects through too many trials all at once. Dr Humphrey felt that almost anyone could get results if only he would follow the clues obtained from earlier work as to what factors may influence success.

The question uppermost in my mind after my visit is this : If we had, in a British Laboratory, a group of experimenters working along the same lines as Duke, would we get the same results?

II. THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

My first introduction to the A.S.P.R. was a short stay with our own president, Dr Gardner Murphy. Dr Murphy, one of the world's leading experts on personality psychology, was president of the American Psychological Association in 1943-4, and since 1940 has been head of the Psychology Department at the City College of New York. Since 1941 he has been vice-president and chairman of the Research Committee of the A.S.P.R. The prestige lent by his name, and his active help and guidance on all research matters, have been largely responsible for raising the standard of research of the American Society and for generally improving its status. At one time the A.S.P.R. was known as the chief sponsor of 'Margery', the alleged physical medium, and many articles of interest to survivalists appeared in the A.S.P.R. *Journal*, but in the last eight years a change has taken place in the

direction of research and more attention has been paid to E.S.P. experiments.

For Dr Murphy psychical research has been a life-long pursuit. His interest was first aroused by family discussions on the medium Mrs Piper, whose attorney was his uncle. Since those early days he has carried out innumerable experiments. When I saw him he had just completed an elaborate test with a group of dowzers operating in a prepared area in Maine. The dowzers, he told me, achieved about as much result as might have been obtained from sheer guessing, and were considerably less successful than some geologists operating under the same conditions. In my view, however, Dr Murphy's most important contribution in recent years has been to guide and inspire the work of junior investigators, many of whom have reason to be more than ordinarily grateful to him.

The Research Associate of the A.S.P.R., Mrs Laura Dale, works at the headquarters of the Society, which consist of some small ground-floor offices on Fifth Avenue. Mrs Dale has been with the Society some eight years, and her name will be familiar to students of psychical research as a regular contributor to the A.S.P.R. *Journal*. She has a great admiration for the high traditions set by the founders of the S.P.R. in London, but her main interest is in quantitative investigation. With Ernest Taves and Dr Murphy she conducted a study of decline effects in E.S.P. results.¹ She has also engaged in a large-scale investigation for the purpose of discovering a consistently successful E.S.P. subject. Upwards of four hundred college students were tested, but with the exception of Lillian Levine, a girl who showed a startling initial effect (see Dr Murphy's Presidential Address²) the results were more or less null. Mrs Dale's best-known experimental project was her dice-throwing tests of PK. Her first series produced positive results, as regards both an excess of willed target faces and the production of the well-known decline effect. In a later series carried out in collaboration with Dr Joseph Woodruff significant results were not obtained.³

Dr Woodruff, whom I was also able to meet, is now working in the Psychology Department at City College. He and Mrs Dale

¹ Ernest Taves, Gardner Murphy, and L. A. Dale, 'The Midas Touch in Psychical Research', *Jnl. A.S.P.R.*, vol. 37, no. 2 (April 1943) pp. 57-83; *ibid.* no. 3 (July 1943) pp. 111-18; *ibid.* vol. 38, no. 3 (July 1944) pp. 164-70.

² *Proc. S.P.R.*, vol. 49, pt. 177 (1949) pp. 1-15.

³ L. A. Dale, 'The Psychokinetic Effect', *Jnl. A.S.P.R.*, vol. 40, no. 3 (July 1946) pp. 123-51. L. A. Dale and J. L. Woodruff, 'The Psychokinetic Effect: Some Further A.S.P.R. Experiments', *ibid.* vol. 41, no. 2 (April 1947) pp. 65-82.

had just completed a series of E.S.P. tests in which they were attempting to correlate the attitude of the subjects to the experimenter with the scores they obtained.

Mrs Dale is Executive Secretary to the Medical Section of the A.S.P.R. which was formed in 1948. Several prominent psychiatrists, including Dr Jan Ehrenwald, Dr Jule Eisenbud, Dr Pederson-Krag, Dr Emanuel Schwartz, and Dr Montague Ullman play an active part in the work of this Section. Dr Eisenbud is particularly interested in the psychiatric significance of apparently telepathic dreams.¹ Dr Schwartz is chiefly concerned at the moment with a new approach to the study of spontaneous cases.² He believes that too much attention has been concentrated upon evidential values, and considers that reported experiences should be studied by the same methods as natural psychological phenomena, and that their characteristics and the characteristics of those who have the experiences should be the primary objects of investigation.

With the president of the A.S.P.R., Dr G. H. Hyslop, who is by profession a neurologist, I had an interesting talk about mediumistic researches. Dr Hyslop expressed the view that the present scarcity of good mediums was probably largely due to the absence of investigators prepared, as was his own father, to devote a very long time to training the medium for useful work. Dr Hyslop regretted that he did not have more time himself to give to research. Nevertheless, the A.S.P.R. is indebted to him for a great deal of help in administrative matters.

Mrs Allison, the Secretary of the Society and chairman of the Publications Committee, has many personal ties with the S.P.R. She was in this country on several occasions for sittings with the medium Mrs Leonard and has been a contributor to our *Proceedings*. She had many interesting facts to tell me about the history of the American Society.

While in New York I visited City College where Dr Gertrude Schmeidler works. She is a clinical psychologist who was attracted to psychical research after attending Dr Murphy's seminar at Harvard in 1937. For a year she was Research Officer to the A.S.P.R. Her work has been concerned mainly with the correlation of subjects' E.S.P. performances with their acceptance or non-acceptance of E.S.P. ('sheep-goat' classification) and their

¹ J. Eisenbud, 'The Dreams of two Patients in Analysis Interpreted as a Telepathic *Rêve à Deux*', *Psychoanalytic Quarterly*, vol. 16, no. 1 (Jan. 1947); 'Analysis of a Presumptively Telepathic Dream', *Psychiatric Quarterly*, vol. 22, no. 1 (Jan. 1948) pp. 103-35.

² E. K. Schwartz, 'The Study of Spontaneous Psi Experiences', *Jnl. A.S.P.R.*, vol. 43, no. 4 (Oct. 1949) pp. 126-36.

personality type as assessed by the Rorschach Projection Test and the Picture Frustration Test of Rosensweig. Throughout this very interesting and important work she has had the benefit of the advice and collaboration of Dr Murphy. Dr Schmeidler gave a brief summary of her various experimental reports to the Washington Symposium in 1948.¹ She also had the unusual honour of delivering a paper on the subject to a meeting of the American Psychological Association at Denver this year. It is unnecessary to give any account of this work here, since Dr Schmeidler has kindly consented to contribute an article on the subject to this *Journal*.

While in New York I was able to meet Mr S. David Kahn, a very keen young experimenter, who is president of an undergraduate society for parapsychology at Harvard. This body had just completed two interesting series of experiments, of which the first concerned the use of an automatic test-scoring machine for E.S.P. experiments. As a labour-saving device the machine was hardly successful. There was a fixed target pattern at which a large number of subjects aimed, and this necessitated a very laborious statistical correction which was carried out by the workers at Duke.² Significant results were obtained, however, under as stringent conditions as anyone could wish. The second series was an approximate repetition of the Schmeidler 'sheep-goat' work. Only a small number of subjects were used, but so far as they went the results tended to conform to the difference in scoring levels predicted by Schmeidler.

¹ G. R. Schmeidler, 'Personality Correlates of E.S.P. as shown by Rorschach Studies', *Journal of Parapsychology*, vol. 13, no. 1 (March 1949) pp. 23-31.

² S. D. Kahn, and U. Neisser, 'A Mechanical Scoring Technique for E.S.P.', *Journal of Parapsychology*, vol. 13, no. 3 (Sept. 1949) pp. 177-85.

‘AN ADVENTURE’

A NOTE ON THE EVIDENCE

BY W. H. SALTER

IN THE year 1911 there was published the first edition of a book *An Adventure*, in which two English ladies, Miss C. A. E. Moberly and Miss E. F. Jourdain (called in that edition ‘Miss Morrison’ and ‘Miss Lamont’) gave an account of a visit paid by them to Versailles in August 1901, during which they saw and were spoken to by persons whose appearance and manner seemed to them peculiar. In 1902 Miss Jourdain paid a second visit, also related in the book, when she had other experiences that struck her as exceptional.

In the winter of 1901–2 they began to read up the history of the period of the French Revolution, particularly in connection with the Trianons, and later they made diligent researches in the French archives and elsewhere. As the result of their inquiries they came to the conclusion that the buildings, objects, and persons which they had seen on 10 August 1901, and those which Miss Jourdain had seen on 2 January 1902, were parts of an eighteenth-century rather than a twentieth-century scene, reproducing Marie Antoinette’s memories of a place dear to her after she had been removed from it for ever.

If these conclusions were sound, the authors had indeed had a most remarkable experience, much the most striking instance of paranormal retrocognition on record.

The authors occupied high academic positions, Miss Moberly having been since 1886 the first Principal of St Hugh’s College, Oxford, in which post Miss Jourdain succeeded her. They were highly intelligent and much respected by a wide circle of friends. Their historical researches were pursued with great ability and perseverance. The book, when first published in 1911, attracted much attention, and several editions and reprints have since appeared. It has frequently been cited in books and papers dealing with psychic phenomena, particularly when paranormal cognition of events in time other than that of the percipient is under discussion.

And yet many psychical researchers have always treated the book with a chilly reserve that found expression in the review of it in the Society’s *Proceedings*.¹ How is this to be explained? Certainly not on the hypothesis that the whole thing is a fake. It is plain from the published documents and from those, still un-

¹ vol. 25, pt. 63 (1911) pp. 353–60.

published, in the Bodleian Library that the authors believed that they had had an odd or uncanny experience, and further believed that their researches proved this to have been historically connected with eighteenth-century scenes and events, many of which were outside their normal knowledge.

In the comments that follow I shall try to explain the initial difficulties which confront a student desirous of getting a clear picture of the case, difficulties due to the way in which the experience has been recorded and put before the public. My comments are based on (a) the various editions of the book, (b) letters and notes of interviews in the S.P.R. archives, and (c) such of the documents in the Bodleian as I was able to see on a visit in 1935, and others inspected later by a friend at my request. No exhaustive review of the case is intended.

Some of the facts shortly to be stated do not seem to have been known to writers who have accepted the authors' interpretation of their experience.

It was on 10 August 1901 that the two authors first visited Versailles. Some time in the following week Miss Moberly 'began writing a descriptive letter of our experiences of the week before', and it was then that she and Miss Jourdain confided to each other that they thought the Petit Trianon to be haunted (1st edn., p. 11). This 'descriptive letter', if it could be found, would be a most valuable document, as it seems to have been the only memorandum made by either of them for three months.

Among the documents deposited by the authors in the Bodleian are some letters passing between them in November 1901. On the 12th of that month Miss Jourdain writes that she was 'beginning to put down' her account of 'our Versailles ghost-story' when a French lady told her that according to tradition Marie Antoinette and other members of her Court haunted the place: see 1st edn., pp. 21-2. (The 10th August was the day in 1792 when the Paris mob sacked the Tuileries). Miss Moberly completed and signed her account on 25 November, 106 days after the experience, Miss Jourdain on the 28th. Miss Jourdain added a note that her account 'was written before seeing Miss Moberly's account'. These are the first accounts, so far as is known, now in existence of the authors' experience of 10 August 1901. I will call them M1 and J1 respectively.

In cases of spontaneous paranormal perception one must have three 'scenes' clearly defined: (1) the *visionary* scene, i.e. the persons, material objects, etc. which the percipient seemed to hear or see during his experience; (2) the *actual* scene, i.e. the persons, material objects, etc. which were, or might have been, perceived through the normal senses by other persons at the same time and

place ; and (3) the *distant* scene, i.e. the persons, material objects, etc. which were, or might have been, perceived through the normal senses by other persons at some different time and/or different place. Unless the visionary scene materially differs from the actual scene, and at the same time materially resembles the distant scene in points not normally known to or inferable by the percipient, no case for paranormality can be made out.

Written records of experiences should be made at the earliest date practicable. Only in that way is it possible to prevent blurring of memory, especially when the experience is complex, and the risk of the record being coloured by knowledge of the distant scene acquired by the percipient between the experience and the date when it was recorded. The knowledge gained from Miss Jourdain's friend that there was a tradition of the Petit Trianon being haunted may quite possibly have influenced the authors, however slightly, in drafting their narratives.

A more serious matter is that in this particular case the visionary scene, the actual scene, and the distant scene, so far as any of the three can be defined, resemble each other in broad outline. The principal buildings, the general lay-out of greenery, paths, and ornamental water were much the same in 1789 and in 1901. It is with regard to minor changes in architecture and landscape gardening, and more especially with regard to the *dramatis personae*, that material for distinguishing the three scenes must be sought.

There have been four editions of the book. The first appeared in 1911, the second in 1913, the third in 1924, and the fourth in 1931 (reprinted 1932, 1934). The documents M1 and J1 are not reproduced in the first, third, or fourth editions of the book. In each of those editions two other accounts of the experience of August 1901 are put before the reader, which I will call M2 and J2. In the second edition,¹ and in that edition only, all four accounts, M1 and J1, M2 and J2, are reproduced. Members who have not the second edition, the S.P.R. copy of which has been abstracted by some unknown borrower (may this catch his eye and smite his conscience!), may compare the four narratives by inspection of the S.P.R. archives in which typewritten copies of them are pasted on cardboard in parallel columns.

It is mainly on the authority of M2 and J2 that the case has usually been judged, and it is accordingly important to know the date and history of these accounts. M1 and J1 were, according to a statement made by Miss Moberly in April 1911 to Miss Alice

¹ In that edition, the authors' real names having not then been disclosed, M1 is called A1 ; J1, A2 ; M2, B1 ; and J2, B2. Now that the real names have been published it is less confusing to use the initials of their surnames, with 1 and 2 to distinguish the order of composition.

Johnson, then research Officer of the S.P.R., 'written to each other, who knew every detail of the scenery', or as stated in the second edition, 'for the purpose of finding out whom we had seen in common'. M2 and J2 were, according to the second edition, 'of a more descriptive character and were written for those who had not seen the place . . . It was not until 1904, on discovering the changed aspect of the grounds, that we attached any importance to B1, 2 papers [i.e. M2 and J2]. They were copied (with introductory sentences) into an MS book in 1906, and then destroyed'.

The first point to note here is that, as M1 was written for Miss Jourdain, and J1 for Miss Moberly, it must be presumed that each read the other's first account before compiling her own second account. This presumption is, as we shall see, supported by the internal evidence, there being various points in which M2 and J2 resemble each other while differing from M1 and J1 respectively. In the advertisement of the first edition it is said that 'the book contains independent accounts by the two authors', a statement hard to reconcile with the facts.

The reason for compiling M2 and J2 is stated to have been to write a more descriptive account 'for those who had not seen the place', and this was doubtless the authors' intention. But they went far beyond it, as may be seen by comparing the descriptions of the persons seen, the second accounts varying considerably from the first. Thus the two men 'who appeared to be gardeners' of M1 become 'really very dignified officials' in M2. J1 mentions a woman and a girl seen together without describing their dress: in J2 it is said, 'I particularly noticed their unusual dress', and this takes about thirty words to describe. Next, there is the man seated by the Temple de l'Amour (or Pavillon de Musique or whatever it was); M1 described his face as 'most repulsive': M2 says 'His complexion was very dark and rough'; J1 says 'his expression was very evil', J2 adds that his face 'was marked by small-pox: his complexion was very dark'. Immediately after this is the incident of the running man, described in M1 as 'apparently coming over the rock (or whatever it was)': in M2 it is said that he 'had apparently just come either over or through the rock (or whatever it was)'. In M1 Miss Moberly 'could not follow the words he said'; in M2 she gives him fourteen words with comments on his accent, on which J1 had already remarked.

Up to this point the two authors had seen the same six persons, though not in the same order. But near the Petit Trianon building Miss Moberly saw a woman sketching, whom Miss Jourdain did not remember seeing. M1 describes this lady and her dress; M2 gives a rather fuller description, and adds that she thought 'her dress was rather old-fashioned and unusual'. Last there is

the young man (M1) or boy (J1) who came out of the Petit Trianon. M2 adds that he came out 'banging the door behind him'; J2 also adds the slamming of a door. The significance of this is that he appeared to come from the direction of a door that was bangable in the eighteenth century, but had been permanently closed for some years in 1901.

It will be seen that as regards all the eight persons mentioned by Miss Moberly, and the seven mentioned by Miss Jourdain, alterations are made in M2 or J2 or both, as regards the descriptions of their appearance or behaviour, and that the alterations are in each case such as to make it more difficult to fit the persons into the actual scene of 1901.

According to statements made in the second edition, 'B1 [M2] was dated November and B2 [J2] December 1901': this corresponds approximately with statements made to Miss Johnson in 1911. But in this matter the authors' memory seems to have been at fault. In October 1902 they wished to interest the Society in their case, and sent it M1 and J1: this is stated in the second edition of the book. These documents were examined by Mrs Sidgwick, whose brother-in-law, Arthur Sidgwick, a prominent figure in Oxford life, had told her of the case. She did not think there was enough in M1 and J1 to serve as a basis for an investigation. Had the much fuller accounts given in M2 and J2 then been in existence, it is reasonable to suppose that they would have been shown to Mrs Sidgwick, together perhaps with M1 and J1 and an explanation of the relation between the two sets of documents.

On 2 January 1902 Miss Jourdain paid her second visit to Versailles. This convinced her that the building where they had encountered the sitting man was not the Temple de l'Amour, as they had called it in M1 and J1. Her report of her January visit, and the pictures she brought back with her, convinced Miss Moberly on this point. In M2 and J2 this name is dropped and the building is described without any specific name being attached to it. The descriptions do not entirely fit any building now on the site, nor any building that the authors can show ever to have existed on or to have been projected for the site. They do suggest a composite recollection of the octagonal, closed-in building now standing on this site and known as the Belvedere or Pavillon de Musique, and the round cupola supported on classical columns known as the Temple de l'Amour, which stands on another site a little way off.¹ That the authors' sense of direction was poor might have been inferred from their failing at the outset of their experience to find the direct route to the Petit Trianon; see 1st

¹ Photographs of both these buildings are reproduced between pages 358 and 359 of vol. 25 of S.P.R. *Proceedings*.

edn., p. 3. The apparent confusion between two buildings on different sites is further evidence of this, and of defects of memory natural after the lapse of time between August and November 1901.

The destruction in 1906 of the originals of M2 and J2 has deprived us of the possibility of testing the dates claimed for these documents by observing whether they showed any of those signs, such as interlineation, change of ink, etc., from which it can often be inferred whether a document has been composed all of a piece or not. It is not very easy to follow the authors' reasoning as stated in the second edition. Documents to which they say they did not attach 'any special importance' were preserved from November–December 1901 until 1904 (? July : see p. 104 of 1st edn.,) when they discovered 'the changed aspect of the grounds'. In 1904 they began their researches in the French National Archives, the result of which seems to have been to make their narratives all the more important, and then in 1906 they copy them into a MS book and destroy them. It is such an odd sequence of events as to suggest that the authors' memory of the dates of composition of M2 and J2 had in the interval between 1906 and 1911 become very hazy. Whether that is so or not, the destruction of original documents later to be published as the foundation of an extraordinary, supernormal experience reflects oddly on their standards of evidence.

All arguments as to date would have been made unnecessary if the authors had dated *all* their accounts, first thoughts and after-thoughts, and had got an independent person to initial them with the date on which he saw them. The authors did indeed give verbal accounts of their experiences to several friends. The value to be attached to statements by persons who say they remember hearing a certain narrative at a certain time depends in general on two things : (1) the amount of detail in the narrative, and (2) the length of time between the date when they heard the narrative and the date when they state they remember having heard it.

The narrative in *An Adventure* contains a mass of details, and it is in the details that the whole point lies, whether it be descriptions of the dress, personal appearance, behaviour, and speech of the eight persons met, or the various features of architecture and landscape gardening. Several of the letters seen by me in the Bodleian were dated 1911 and 1912. After an interval of nine years and upwards, few peoples' memories of a complicated narrative are to be trusted. What is the earliest, independent, unambiguous evidence, not for *a* narrative of Versailles, but for the particular narrative set out in M2 and J2? It is for the authors and their editors to produce the evidence they rely on. Vague references to papers in the Bodleian are not good enough.

By 1906, when, the authors state, the originals of M2 and J2 were copied into the MS book (now in the Bodleian) and then destroyed, the authors' knowledge of the Petit Trianon of the revolutionary period was much enlarged. They had been reading up the period since the winter of 1901-2 and their systematic searches in French archives began in 1904.

Many people who have attempted to write out an account of a complicated occurrence, whether normal or not, which includes several incidents, will by inadvertence, even while their memory is fresh, omit details which on reflection they will wish to include in their record. It is proper to record these afterthoughts, but to record them as such, so that there shall be no confusion between them and the record as originally made. The afterthoughts should be dated and independently attested, so that they may be judged on their own merits.

There is accordingly lacking in the Versailles case what should be (I repeat, *should be*) the starting point of any case of paranormal perception, namely an unambiguous description of the visionary scene recorded in such a way as to exclude faults of memory. It is not therefore possible to effect a clear comparison of the visionary scene of this book with either the actual or the distant scene. As to the distant scene, *An Adventure* suffers, in comparison with other cases of paranormal perception, from a peculiar difficulty. In a typical case the distant scene is objective, and the circumstances composing it can be ascertained by inquiry. The percipient, say, while sitting in his office has a vision of his brother being knocked down by a car in another town. In a case of that type it is usually possible to ascertain whether such an accident has happened, and if it has, to discover just when and where and how. In the Versailles story the distant scene is supposed by the authors to represent the memories of Marie Antoinette. That supposition of the authors is not based on anything in the experience as they recorp it. Their diligent research enables them to say that such and such persons *might* on some occasion have been grouped together in her thoughts, but not that in fact they were. The distant scene is therefore in this case both subjective and hypothetical.

There is also much vagueness as to the third of the relevant scenes, that which has been called the actual scene. It has long been recognised that the possibilities of a normal explanation are much greater when a supposedly paranormal perception occurs out of doors than when it takes place in a room. It is usually possible to observe whether figures seen indoors come and go as creatures of flesh and blood, or appear and disappear as phantoms. The number of ordinary folk likely to be seen in a room at any particular time is limited, and the identity of any who were in

fact present can often be ascertained. A public park is, evidentially, about the worst setting for a ghost-story, especially if the supposed ghosts can only be identified from descriptions made months, or possibly years, later.

No one can say what persons, besides the authors, were in the gardens of the Petit Trianon on 10 August 1901 during the time of their visit. Perhaps if they had immediately issued an advertisement inviting the man with a pock-marked face and a repulsive scowl who was sitting that afternoon on the steps of the Pavillon de Musique, (or was it the Temple de l'Amour?) or the lady with an old-fashioned, rather dowdy dress (see 1st edn., p. 75) and an unattractive face, who was sketching near the Petit Trianon about the same time, to communicate with them, some contemporaries might have come forward in the flesh claiming to answer those descriptions. By 1911 all that was possible was to consider whether the persons described were the sort of people who might naturally have been met in the Versailles of 1901.

It should be remembered that in 1901 the French were far from Anglophile, and that to them the English spinster wandering unescorted on the Continent was an irresistible joke. Perhaps it is not necessary to look further for explanations of the scowls of the sitting man (M2), the 'peculiar smile' of the running man (M2), or the 'peculiar smile of suppressed mockery' of the lad who banged the door (J2).

Were the authors right in supposing that the dress of the persons seen was not that of 1901? I shall not hazard an opinion as to whether the dress of the lady seen sketching might or might not have been worn by a woman artist in 1901, or whether it would have been 'unusual' for a girl of 13 or 14, the daughter, it would seem, of a local employee, to have worn a dress reaching to her ankles in the Versailles gardens of that period, as described in J2. 'Unusual' it would have been in the England of 1901, but not at that time in several country districts on the Continent. As to the men, the uniform worn by the 'gardeners' does not seem to have been that of any persons employed in the gardens in 1901, but is it quite certain that it was not the uniform of other minor functionaries on a visit? France is full of all sorts of uniforms, and Versailles attracts visitors from all over the country. The cloaks and sombreros (or slouch hats) of the sitting and running men were, unless my recollection of that period is wholly wrong, an attire much affected by contemporary artists.

Nothing is said by either of the authors in either of their accounts as to what any one of the five male persons, four men and a lad, wore on their legs. This is a matter in which the difference between 1792 and 1901 would leap to the eye of an observant

person. Roughly speaking the difference is between breeches, the general wear of all ranks in 1792, and trousers, the universal wear in 1901, except for special occasions, such as Court Dress or sport. The silence of the authors on this crucial matter suggests that they observed no masculine leg-wear inappropriate to 1901 because there was none to observe. If that is so, then there is a strong presumption that all the male *dramatis personae* of 1901 were persons of the twentieth century, and inferentially that the females were so too.

M. Sage, an Honorary Associate of the S.P.R., who was a Frenchman and knew Versailles well, gave it as his opinion in 1911 that all the supposed eighteenth-century persons described in the book might well have been met in the flesh in the Versailles of 1901.

I have no desire to attempt to prove that Miss Moberly and Miss Jourdain had no remarkable experience on 10 August 1901. They thought at the time that they had had one, and when intelligent people in good health think that, they probably have. There are, however, many different kinds and gradations of experiences, from vague to precise, from purely subjective to veridical. But the authors recorded, investigated, and published their experience in such a way as to leave the whole affair in an impenetrable fog of uncertainty. All this would have been avoided if they had added to their many virtues some knowledge of the standards of evidence, and the recognised procedure for conforming to them, that the peculiar subject-matter of psychical research makes necessary. Contemporary records properly dated, independent attestation, careful inquiry, before any other research was begun, as to whether a normal explanation was possible—these were indispensable preliminaries for an investigation which, in view of the nature of the experience and the conditions in which it occurred, was in any event bound to be difficult. Through their failure to take these steps, their elaborate researches, conducted with an ability and perseverance worthy of all praise, rested on an insecure foundation. Their knowledge of French literature should have reminded them of the comment made by a French lady when told of an earlier and hardly less notable paranormal experience reported from the neighbourhood of Paris, 'C'est le premier pas qui coute'.

The differences between M1, J1 and M2, J2, and the uncertainty as to the dates of the latter two documents have been discussed at length, because these are matters on which few persons who have heard of the Versailles adventure are well informed. The facts were known to the officers of the Society in 1911, but as their knowledge had been obtained confidentially, they asked the authors' permission to compare the two sets of narratives in the review which was being prepared for *Proceedings*. This was

refused, and the reviewer (Mrs Sidgwick) was in consequence severely handicapped. With the publication of the second edition, this limitation on the freedom of discussion disappeared, but the refusal having once been made, the S.P.R. could not have re-opened the case without saying things that might be considered offensive to two elderly and much respected ladies. As the book still attracts attention, the time has come when it can without offence be pointed out how two extremely able women, starting out with the best intentions, muddled their case at an early stage so completely as to make all their later labours useless : and all because they had not joined the S.P.R.! Even if M2 and J2 are allowed the dates the authors claim for them, there remain formidable difficulties in the way of accepting their interpretation of their experiences. How exact was their memory? Or their sense of direction? How sharp their powers of observation? What was their knowledge of contemporary French life? Were they free of bias in favour of supernatural explanations? These are matters to be examined in the light of the evidence the authors have given us.

REVIEWS

IN MY MIND'S EYE. By Frederick Marion. With a Foreword by R. H. Thouless and B. P. Wiesner. London, Rider, [1949]. 272 pp. 18s. Illus.

This book is the autobiography of Josef Kraus ('Marion') a well-known vaudeville telepathist. Marion, a Czech, was born in Prague on 15 October 1892 and was educated at the Commercial Academy in the same town. He describes how he was launched on his stage career by his spectacular success in locating certain objects which had been hidden in various parts of the city of Prague by a committee composed of police and other townsmen. This led to music-hall engagements at first in Prague and afterwards in various Continental towns such as Vienna, Budapest, Krakow, Berlin, Cologne, etc. At these performances Marion mystified his audiences by finding objects hidden by them in various parts of the hall, by guessing numbers and colours chosen by members of the audience, and by describing events in a person's life by scrutinising a specimen of his handwriting. This, of course, is the usual stock-in-trade of the stage 'occultist', but Marion's work cannot be dismissed quite so lightly as that of the Piddingtons or the Zomahs for he does not, to the best of my knowledge, employ either codes or confederates.

His methods resemble those of Maloitz, the Dutch stage performer mentioned by Mr Harry Price in his *Confessions of a Ghost-Hunter*. Both Marion and Maloitz are experts in the interpretation of indicia obtained by watching the reactions of the audience.

Marion devotes several chapters to the strange adventures which he encountered as a music-hall entertainer. There is one inimitable story describing how he was offered an engagement at the town of Perm on the borders of Siberia. When he duly arrived at his destination with half a ton of stage trappings, he found he was called upon to perform to an audience of one and the audience was sitting with its back to the stage. Somewhat disconcerted, Marion began with his usual stage patter about telepathy. As he proceeded the 'back' turned slowly round, its face began to smile, then it chuckled, and finally roared with mirth. Then before Marion had time to do a single experiment the audience—who proved to be an elderly gentleman with aristocratic-looking features—called him across the room and thanked him for the finest comic performance it had ever witnessed in its life. It seemed that the gentleman was no less a person than a Russian grand duke, in fact the exiled uncle of the Tzar Nicholas. This extraordinary performance was repeated each night for a fortnight without the least variation.

The book is written in the chatty, pleasant style that is reminiscent of Harry Price's *Confessions of a Ghost-Hunter* and it contains even taller stories than that masterpiece. This highly-coloured journalistic kind of writing has the drawback that it gives any work an anecdotal flavour which prevents one from taking any of the incidents very seriously. It is unfortunate, but it is not easy to be sensational and accurate at the same time. One marvellous story follows another without a shred of corroboration or supporting evidence. Thus (p. 143) Marion tells Dr Springer, a Nazi, that he, along with the upper ten of the Nazi Party, will hang on Marion's birthday, 15 October. This prediction, according to Marion, was made at Dresden in 1931. On another occasion he used his 'powers' to assist a woman to recover a currency note which it turns out had been purloined by a milkman. If even half these stories are true Marion must be the greatest psychic who has ever lived. But one feels strongly that even were an investigation set on foot to discover the truth about these claims it would fail owing to the people concerned being either dead or untraceable.

Marion has also dabbled in criminology. The bulk of his stories are as usual uncorroborated, but in one case he does produce a translation of a letter he received from the Royal Dutch Constabulary at Apeldoorn after carrying out a test. Marion was handed a small box containing an automatic pistol which had been

used for a crime. Of this crime none of the officers present had any knowledge. Marion stated correctly that three shots were fired by a man at a woman with this pistol but that the woman was not killed or fatally injured. He gave many other details, but it was impossible to verify them. Marion's description of the man could not be checked by comparison with a photograph on a police index card.

What is certain is that when Marion visited England in 1934 and was tested by the writer and a group of other persons at 13d Roland Gardens, the offices of the University of London Council for Psychical Investigation, his remarkable powers must have been entirely dormant. As I have described in my monograph *Preliminary Experiments with a Vaudeville Telepathist*, numerous people handed Marion objects, letters, and specimens of handwriting on which to exercise his skill, but the 'readings' he gave were evasive and vague, devoid of specific detail, and banal to the last degree. Not one of the persons who tendered the objects was convinced. When handed part of a letter from a woman Marion described the writer as a man. When he was given a letter from a mischievous boy of fourteen he said the writer was an elderly *savant* aged about fifty who worked in a large institution and was of a cantankerous nature. He also claimed that by inspecting a person's handwriting he could tell whether that person was alive or dead. We tested him with a carefully controlled statistical experiment and he failed to substantiate the claim. Of the present reviewer's brother who was killed in the First World War at the age of 19 Marion asserted: 'This writer is a middle-aged man, good-hearted, and likes a quiet life. Living'. So much for Marion's claims as a graphologist.

In his autobiography Marion tells us for the first time that he was employed by the Austrian army during the First World War as an expert water-diviner in Albania. It is rather curious that when he came to be investigated at Roland Gardens he never revealed this fact to us. We should have been delighted to have carried out experiments along these lines. On page 20 Marion makes a very unusual claim. He writes: 'I have carried out many times an experiment in which I contrive to shatter a thin wine-glass by projecting thought-waves upon it. Incredible? Not at all.' I suggest that Marion should demonstrate this to Dr Rhine. Surely it would be a more convincing proof of psychokinesis than all the dice-throwing that has ever been done in the Parapsychology Laboratory at Duke University!

Marion honours me with a whole chapter devoted to the tests I carried out with the assistance of Mrs K. M. Goldney and others during the spring and summer of 1934. The report of these experiments is contained in Bulletin 3 of the University of London

Council for Psychical Investigation (London, 1937). I carried out a series of further experiments in the years 1938-9, but these are still unpublished. Marion does not agree with any of my conclusions, and in his chapter 'On Trial in England' he quotes numerous extracts from a criticism written by a certain Mr Edmond P. Gibson who has had some association with the Duke University workers in parapsychology. Many years ago Mr Gibson sent me a typescript which I thought at the time was the most extraordinary piece of special pleading I have ever read. I answered Mr Gibson's points in great detail, but received no reply from him. Mr Gibson, who is apparently annoyed because my experiments showed that Marion does not use telepathy in his stage feat of finding objects hidden by members of the audience, questions the validity of my methods. Let me now put the plain facts before the reader as succinctly as possible.

In order to test Marion's ability to find hidden objects we placed six tin boxes in different parts of the room. Marion was taken outside the room by another person. In his absence a die was thrown in order to decide in which of the six boxes a light scentless handkerchief was to be hidden. The lids were replaced on the tins and each tin was given a random push. The audience, consisting of five or six persons, then seated itself round a table. Marion was called in. The rule was that if he touched a tin he must open it. Members of the audience were not to give him any deliberate clues. Marion walked round the room and ultimately opened a box. In a total of 91 trials in which Marion's friend Mr Van Lier was excluded Marion opened the right tin no less than 38 times and the odds against such a result being due to chance are nearly one in seventy-one millions. Obviously, then, Marion could perform this feat. But observe the sequel which Mr Gibson blatantly ignores. A curtain was rigged up across a corner of the room. In this curtain were a few tiny chink-holes. In the new experiments, immediately the handkerchief was hidden the audience retired behind the curtain and Marion was called in. The audience, who all knew in which tin the object was hidden, watched Marion through the chink-holes, being careful not to disturb the curtain. The audience 'willed' Marion to go to the right tin as they had done in the previous tests. The only difference was that now Marion was unable to observe the audience or any of their head-movements, etc. Four afternoons were devoted to this series, and Marion opened the correct tin only 13 times in 64 trials. The chance of getting such a result is greater than 1 in 4. If Marion, as he claims, performed his feat by telepathy, why should he fail so completely when the audience was still in the room but invisible to him?

In the face of such a finding I simply cannot imagine why any person should want to maintain that Marion uses telepathy.

In another test the audience was seated round the table, but each person had a thick blanket over his head and shoulders and stopped his or her ears. Marion, under these conditions, was right just 4 times in 28 trials, a result which we should expect by chance alone.

In one experiment in which Mr H. S. Collins, who knew in which tin the object was hidden (the rest of the audience being behind the curtain), was seated in a corner of the room with a stockingette hood over his head, we watched Marion moving his hand up and down over a tin deliberately waiting for Collins to nod his head or give some other tell-tale sign that Marion was at the right tin. Marion kept glancing repeatedly at Collins. We noticed the same thing in other experiments when Collins was standing in the sentry box with only his hooded head exposed.

Mr Gibson makes great play with the word *hyperaesthesia*. But in the conclusions at the end of my report I do not even mention the word. In the above feat there is no question of hyperaesthesia. It is merely a matter of Marion's trained observation. Marion, by years of practice, has trained himself to *interpret* correctly small movements on the part of those who know where the object is hidden.

Mr Gibson asks 'Why is Mr Soal so miserly with his perfect experiments?' He says erroneously (p. 226) that I only made 25 trials which I considered to be free from sensory cues. He ignores entirely the 64 curtain experiments which were as 'perfect' as can be desired. These in number compare quite favourably with the 91 experiments in which Marion could watch the movements and reactions of his audience. I had no particular reason to suppose that Marion employed genuine telepathy in his stage work. I soon satisfied myself that he did not. Only the credulous believe that vaudeville mystery showmen perform their feats by telepathy, and why should Marion prove an exception? My main object was to gain some idea of the perfectly normal methods used to produce certain effects. 'Perfect' experiments which made it impossible for Marion to succeed at all would have taught me just nothing of the *modus operandi*.

Mr Gibson's views seem to be in a minority of one. Such scientific journals as *Nature* and the *British Medical Journal* commended my work in the highest terms and the first-named journal applied the epithet 'outstanding' to it. Nor would anyone accuse the late Whately Carington of hostility to the paranormal. Yet in his book *Telepathy* he wrote: 'His paper on the subject deserves to rank as a classical example of how such work should

and can be done . . . Soal conclusively showed by a beautiful series of step-by-step tests that this [feat of finding hidden objects] was due to the subconscious utilisation by Marion . . . of trifling clues and indications unwittingly given by members of the audience.'

The only other performance of any interest in which Marion was successful consisted in the picking out of a playing card which he had previously handled after it had been mixed with several cards of similar make and design. It should be emphasised that he was quite unable to say correctly what figure was on the face of a playing card which he had never seen before when merely allowed to see its back. He merely *recognised the actual piece of paste-board* which he had previously touched. If this was 'clairvoyance' it was a very strange sort of clairvoyance indeed! Marion proved himself quite unable to emulate the feats of good card-guessing subjects such as Mrs Stewart or Basil Shackleton. Our experiments showed that Marion used sometimes visual and sometimes tactual cues. He would manage to give the card a slight flex while handling it so that it stood out from the other cards as being less flat when placed on the table. When we gave him a very thick mill-board card that was almost inflexible and then sealed it up in an envelope and mixed the envelope with others which contained similar cards, Marion would spend two or three minutes in vigorously ruffling the *edges* of each envelope so that it was painful to watch him. The only way in which these stiff cards could be recognised tactually through an envelope was by feeling for certain slight ridges and concavities along the edges, and it became quite obvious that Marion was trying desperately to identify the card in this way. Even so, he scored hardly any success. The word 'hyperaesthesia' used by Drs Thouless and Wiesner and Mr Gibson is altogether too strong to use in this connection. Marion possesses a fairly sensitive touch and that is all. He was quite unable to emulate the alleged feat of the blind osteopath, Captain Lowry, of whom it was said that he was able to distinguish the pips of a playing card by touch alone. Mr Gibson makes one remark that it is glaringly misleading. He says: 'Mr Soal argues that each card tapped or touched by Marion has a pitch or keynote of its own, perceptible only to Marion' (p. 227). This was a casual suggestion by Professor Harris the physiologist and not my hypothesis at all. On page 42 of my report I merely mention it and remark that it seemed far-fetched.

Drs Thouless and Wiesner in a foreword to Marion's book state that in their opinion he possesses paranormal capacities of an unusually high order. It is impossible for me to comment on this statement since, in spite of a considerable lapse of time, no report

on their experiments has yet appeared. During the past three years I have heard reports of Marion's methods which are not reassuring. In particular, two members of the Magic Circle watched Marion's trick of picking out a 'red' card from among seven 'black' ones without tactual contact¹ and on 25 March 1947 one of them, using the same pack of playing cards with which Marion had performed his feat, demonstrated before Dr Wiesner, Dr West, and Mrs K. M. Goldney that it was possible to duplicate Marion's feat. Dr Wiesner, Dr West, and Mrs Goldney all signed a statement to the effect that they were completely convinced that the conjuror had made good his claim. Marks were discovered on the backs of the cards, *predominantly though not entirely* on the cards of red denomination. Whether these marks were manufacturers' marks or not is beside the point. If they could have been utilised to identify certain of the 'red' cards, then clearly the experiment was valueless as a test for paranormal cognition.

I might also mention another incident which occurred recently at a party in the house of Mrs C. Richards. During the evening Dr Flugel handed Marion a sealed envelope in which Mrs Goldney had put a letter which Marion was to 'psychometrise' later. On the envelope Mrs Goldney had placed a secret mark. Holding the envelope in his hands, Marion gave a remarkably accurate 'reading' of the contents of the enclosed letter. Mrs Goldney then asked Marion for the envelope, which she insisted on seeing. To her surprise she saw that the envelope was not the original one in which she had sealed the letter. Marion explained to her that he had opened the original envelope by mistake believing that it contained a cheque from Mrs Richards. He said that he had not read the letter but had immediately sealed it up in a fresh envelope.

But surely it was Marion's duty after opening the envelope to have gone to Mrs Goldney or Dr Flugel and explained what had happened. He must have known that once the envelope had been opened the experiment lost its value. Had Mrs Goldney not made the secret mark and insisted on examining the envelope the incident might have appeared as a striking exposition of Marion's *psi* powers. Incidentally, the subject-matter of the enclosed letter was printed in inch-tall letters at the head of the notepaper so that it would have been practically impossible not to read this in transferring the letter from one envelope to another.

In my report on the 1934 experiments I have recorded cases which reveal a similar lack of candour on Marion's part. On 28 February 1934 I decided to put Marion's straightforwardness

¹ Incidentally, this was not part of Marion's stage performance prior to 1934, since it was I who originally suggested he should try these experiments.

to the test. I obtained twelve white cards on which were printed in thick black type the numbers 1, 2, 3 . . . up to 12. I placed each card in a thin envelope and sealed up the envelopes. The envelopes were so thin that when the card fell against the inner face the number on it could be read by a person with normal eyesight. The envelopes were handed to Marion one by one and he wrote on each envelope the number he thought it contained. Only in the case of three of the envelopes was Marion definitely wrong. In two other cases he could not decide whether the number was a one or a seven. We watched him carefully while he was manipulating the envelopes and it was obvious that he was tilting the card against the face of the envelope and reading the number enclosed. But did he say that he was reading through the envelope? He did not, but pretended that he was using clairvoyance. I then handed him twelve *opaque* envelopes containing similar cards numbered one to twelve and presented them to him in random order. Now he did not score a single success at first try and only two at second try, even though he made two guesses at the number in each envelope.

It is, of course, quite legitimate for a vaudeville telepathist to use any method that is available so long as he does not claim paranormal powers. But all through this book Marion insists that his work is not mere entertainment but is of value to science as examples of telepathy, clairvoyance, or precognition. This attitude, I am sorry to say, is inconsistent with our observations of his shifty methods. How can Marion claim to be helping science to study telepathy, when all the time he knows he is using normal perception?

In 1939 Mrs Goldney and I tested Marion with many hundreds of guesses at Zener cards, but under properly controlled conditions he failed to demonstrate any powers of clairvoyance. On the first occasion he made his rectangles and circles look so much alike that in checking up we had to ask Marion to say if a symbol was meant for a circle or a square. Under these conditions he managed to win an excess above expectation equivalent to four standard deviations. But after this we were on our guard and insisted on his identifying the written symbols before he had any sight of the target symbols. Though we went on for many weeks Marion failed to score again above chance expectation. At the same time he claimed that when working alone without witnesses he was getting astonishing scores.

Marion is a genial person with great natural charm, and I have pleasant memories of his hospitality when he lived at Abinger Hammer. He is no conjuror, and his movements are slow and deliberate. Some of his stage demonstrations of his ability to find hidden objects and of kindred feats of pseudo-telepathy are quite

remarkable. I would not care to assert dogmatically that he has never shown any paranormal ability, but it is a pity that he claims that his stage effects are produced by telepathy. It is a still greater pity that while claiming to work by paranormal means he will resort to normal methods without any acknowledgment that he is using such methods. These defects must inevitably impair confidence in any claims made by him to the possession of a genuine paranormal capacity.

S. G. SOAL

ACQUAINTED WITH THE NIGHT. By Nancy Price. Oxford, George Ronald, 1949. 155 pp. Illus. 7s. 6d.

Miss Price's collection of dreams, fresh-frozen on waking, preserves amazingly those flavours of the sinister, the unexpected, and the inevitable which eddy round the caverns of sleep. They are of interest to the common reader because he too has dreams, and these rouse the distant echoes of his own. They should be of value, as raw material, to the psychologist. The subject matter of a very few comes directly into the sphere of psychical research. 'The Red Light' (pp. 104-5) and 'Jumble' (pp. 115-7) contain precognitive flashes, though both are, so to speak, incidental, and the emotional weight of the dreams falls elsewhere. There is, moreover, a particularly interesting passage on pp. 71-3 in which Miss Price records a dream that someone was dying, her recollection of gasping, fragmentary sentences, her awakening to a smell of cigar smoke and to the conviction that someone was in her cottage (a conviction surviving a search which showed there was no-one there), and her hearing next morning of the death of James Agate at the relevant time.

Lord Dunsany contributes a short, pleasant preface, and Miss Price herself a long and fascinating introduction discussing dreams in general, and a number of famous 'warning', creative, and prophetic dreams in particular, among them Lincoln's dream of his own assassination, Williams's dream of the murder of Percival, a dream which is alleged to have saved General Gordon's life, Pharaoh's dreams, and the dreams which have inspired various poems and novels.

RENÉE HAYNES

TIJDSCHRIFT VOOR PARAPSYCHOLOGIE, Vol. 17, No. 4/5, July/September 1949. The Hague, H. P. Leopold's Uitgeversmij. 2 guilders.

Dr W. H. C. Tenhaeff concludes his study of 'Dowsing and Water-divining' with an interesting article dealing with the psycho-

logical and physical problems connected with the zones of disturbance in the earth's magnetic field commonly attributed—at least in Holland—to 'terrestrial radiation'. There is a widespread belief that there is some relation between these hypothetical 'earth-rays' and malignant tumours, and many cases are reported in which dowisers are consulted about sites for houses and structural alterations, especially as regards bedrooms. Dr Tenhaeff considers the existence of the problematical 'radiation' to be *sub judice*, as well as any connection with cases of carcinoma. He stresses the need for an objective investigation by the Netherlands State Dowsing Committee, as the exploitation by quacks of incipient carcinomophobia (in order to promote the sale of 'protective apparatus') is a matter which concerns public health.

In a paper on 'Telepathic Double Dreams' Dr Tenhaeff gives some instances of simultaneous corresponding dream-visions, in which the resemblance between both dreams may be attributed to the lowered threshold of consciousness of both dreamers, one of them acting as agent and the other as percipient, or perhaps combining both functions. This kind of telepathic contact may be thought to be encouraged by libidinous ties (in the analytical sense) between the telepathic partners. Dr Tenhaeff refers to the theories of a collective unconscious (Bergson, Carington, Driesch, Heymans) and to the possibility of individual thoughts cropping up in another individual's consciousness through the medium of this collective substratum. According to Dr Tenhaeff, there is a close analogy between *psi* processes and those of the 'normal' subconscious inter-relations between individuals.

Attacking—in an article entitled 'The Astral World'—Professor Van Os's statement (*Tijdschrift*, Vol. 17, No. 1) concerning the individual's post-mortem situation, Dr M. Lietaert Peerbolte, a Hague psychiatrist, expects the hereafter will present less resemblance to the dream-like state (implying a creative and directive power over the spirit's environment, as in wishful dreaming) than would follow from Van Os's ideas. Founding his belief on observation of spontaneous conversions to 'Cosmic Consciousness', especially among the aged, Dr Peerbolte strongly opposes the 'spiritistic' aspects likely to be associated with Professor Van Os's more individualistic theory. He thinks it incompatible with the results of psycho-analytical investigation and inconsistent with the established fact that (by means of psycho-analytical methods) man may be freed from urges and passions even while living.

Mr G. Zorab describes and analyses a precognitive dream involving an attack with incendiaries and a round-up of Jews in an Amsterdam ground-floor flat. The resemblance is certainly

striking ; but the dream took place at least six years before its supposed fulfilment, giving ample opportunity for chance to operate. Further, in April 1935, when the dream occurred, this sort of incident might well have been foreseen logically. Mr Zorab reserves judgment.

J. C. M. KRUISINGA

CORRESPONDENCE

MR PARSONS'S PAPER 'ON THE NEED FOR CAUTION IN ASSESSING MEDIUMISTIC MATERIAL'

SIR,—It seems clear to me that Mr Tyrrell's criticism is based on a misunderstanding of the purpose of Mr Parsons's experiment. Surely this purpose was not to prove that Mrs Gay's sitting with Mrs Bedford contained no paranormal element, but, by showing that ostensibly paranormal material (presented to readers as a self-contained piece of evidence) can be of so much smaller evidential value than it appears, to demonstrate the need for special caution in judging mediumistic material. Mr Parsons is simply giving us an entertaining and salutary lesson in caution—the old, old lesson of the fallibility of unaided human judgment. Surely that is all there is to it.

The question of selection of evidence is, I think, rather more complicated than Mr Tyrrell suggests. One of the most important principles in science is to select the material on which you are going to experiment. The psychologist, for example, wishing to test the influence of innate factors on intelligence must select a population in which environmental factors are uniform. Similarly, the psychical researcher who wishes to decide between the hypotheses of chance and *psi* must select material in which any third factor, such as rational inference from persons present at the sitting, is ruled out. What the scientist must *not* do is select the *results* of his experiments. Mr Parsons published his whole experiment.

The open-mindedness of Mr Parsons himself is a separate question, and I think we should be extremely careful in discussing it in a scientific context. It is only too easy to take the view that those who disagree with us are prejudiced, and to explain away their views as psychological symptoms instead of discussing them as serious hypotheses. Surely we must concentrate on discussion of methods and results rather than attitudes and motives. We

must devise methods of research whose conclusions are so clear that no-one, whatever his attitude or bias, can question them.

Mr Abdy Collins's criticism seems legitimate, but this is a matter which is best tested by experiment rather than by speculation. With this intention I showed pp. 346-51 of Mr Parsons's article to two professional psychologists who have no views on the reality or otherwise of *psi*-phenomena. I was careful not to suggest any particular conclusions, and to see that they could not tell which group of annotations came from the true subject. I replaced Miss A's annotation on item 16 by 'not placed', and asked them to choose between Mrs G. and Miss A. Using their own systems of assessment they arrived, independently of each other, at the opinion that there was nothing to choose between Mrs G. and Miss A. Mr Parsons was, I think, rather too hasty in his conclusion, but at any rate this little test shows that 'an impartial judge' may not arrive at the same assessment as Mr Collins.

CHRISTOPHER SCOTT

SIR,—I have read with interest Mr Tyrrell's letter in the November issue of the *Journal*. His criticism seems to turn on the point that Mr Parsons, misled by an unconscious urge, only used part of Mrs Gay's sitting for his experiment—the part that had been separately published in the *Journal* for March–April 1947. But surely that part—consisting of items obtained under proxy conditions—was in a different class from the remainder when non-proxy conditions were in force? Now to take both parts, proxy and non-proxy, lumped together for Mr Parsons's test would seem to be as mistaken as neglecting, for example, to take into account the rigorousness or laxity of the conditions in a series of card-guessing experiments. If, say, for the first 500 guesses the conditions are lax, but for the second 500 rigorous, I fail to see that any sure conclusion can be drawn from analysing the *combined* totals of hits and misses.

Mr Parsons might have made a second experiment using non-proxy material only. Had he done so the conclusions to be drawn might have been that while proxy items are of so general a character that any hits are possibly attributable to chance, the better-defined items produced under non-proxy conditions—whether due to spirit guidance, to telepathy from the sitter, or to other less reputable causes—'fit' more accurately and the chance-test tends to be negative.

I agree with Mr Tyrrell that there is a bias in most of us that embarrasses our judgment in paranormal matters. I would like to suggest, however, that when anyone may have crossed the

Rubicon of paranormal belief there is an even more dangerous tendency to relax our scientific caution and accept as evidence material that falls short of the standard under which we began our psychical march. Having swallowed the camel we cease to strain at the gnats—or even other camels!

G. W. FISK

